

molecules of some phosphorescent substance to be in a state of constraint, and to relieve themselves, thus causing vibrations which are communicated to the ether—the whole change taking place so quickly and on so small a scale that the statistical law above-mentioned does not apply, and is not therefore broken. Nay, further, I can imagine an enclosure, the walls of which are coated internally with an excited phosphorescent body performing for all practical purposes the part of an enclosure of low temperature under the theory of exchanges, and yet it may be continuing for some time to emit visible rays.

(2) I can, however, imagine the following question to be put. Let there be a phosphorescent substance which is capable of being excited by certain rays coming from a black body at the temperature T , these rays being apparently converted into others of lower refrangibility which continue to be given out for some time by the phosphorescent body. Let us further suppose that the phosphorescent body does not suffer chemical decomposition at the temperature T .

Now imagine a temperature enclosure kept at temperature T , the interior walls of which are lined in part with this phosphorescent substance. What will happen in this enclosure?

I think there can be little doubt that if there be such an enclosure capable of existing permanently and without decomposition of the substances which compose it, then the rays which it gives out must be those required by the theory of exchanges. But if the further question be asked in what way does the phosphorescent body conform to the theory of exchanges, we may, I think, plead ignorance. As far as I am aware we have experimentally little or no knowledge of what the phosphorescent substance will do under these conditions, presuming that it can exist undecomposed. All our knowledge is limited to its behaviour at a low temperature when acted on by high temperature rays, and its peculiar behaviour under these conditions cannot, I think, be viewed as a valid objection to the theory of exchanges.

BALFOUR STEWART

The Eleven-Year Meridional Oscillation of the Auroral Zone

THIS very remarkable law, in favour of which Mr. Tromholt quotes a short series of observations made at Godthaab, which, he says, are supported by a few in other Polar regions, would, it seems to me, if satisfactorily proved, not only advance the science of terrestrial magnetism a stage, but also materially help to elucidate the exceedingly mysterious bond of union between the aurora and weather. As long as we simply knew that the manifestations of the Aurora Polaris increased and diminished everywhere with the spotted area of the solar surface, we were obliged to conclude that there was a similar increase and decrease in the electrical energy of terrestrial currents, and meteorological evidence did not favour the idea that the eleven-year variation in terrestrial currents was on such an extensive scale as the amplitude of the auroral oscillation would imply. But now if the law which Tromholt has indicated, really exists, a great deal of the difficulty in correlating the two phenomena disappears, since it is obvious that a comparatively small displacement of the zone would cause the annual average number of auroræ to increase or diminish by their normal amount. Thus from lat. 60° N. to lat. 65° N., a distance of only 350 miles, the annual average number of auroræ diminishes from 80 to 40.

I will not now dwell upon analogous eleven-year oscillations of isobars, such as Blanford's Asiatic seesaw, and the indications of similar secular displacements of the Atlantic isobars noticed by Allan Brown and others, or upon the extraordinary resemblance in form between the auroral zone and the mean storm track of the northern hemisphere charted by Prof. Loomis in his latest contribution to meteorology; but I would merely say that Tromholt's discovery seems likely to become the touchstone which may, in the hands of an intelligent and comprehensive worker, clear up the entire question, and I earnestly hope that no efforts will be spared to corroborate it.

I will conclude by adding my mite. In looking over Fritz's monograph on the connection between solar spots and terrestrial magnetism and meteorology, I have found a series of observations at Godthaab and Jakobshavn ($60^{\circ} 22'$ N.) further north, which do not appear to have been utilised by Mr. Tromholt, and which, when combined in the form of percentages, cover a space of ten years, and add strong corroboration to the law indicated by Tromholt.¹

¹ "Über die Beziehungen der Sonnenfleckperiode zu den magnetischen und meteorologischen Erscheinungen der Erde," p. 48.

TABLE I.—No. of Auroræ seen annually at Godthaab and Jakobshavn, compared with Wolf's Sunspot Numbers

Years	1840	41	42	43	44	45	46	47	48	49	50
Godthaab	—	60	93	84	87	74	32	—	—	—	—
Jakobshavn.	10	15	15	18	12	24	21	17	14	11	21
Sunspots	63·2	36·8	24·2	10·7	15·0	40·1	61·5	98·4	124·3	95·9	66·5

TABLE II.—The above numbers of Auroræ converted into percentages of their means and compared after smoothing with smoothed Sunspot Numbers¹

Years	1840	41	42	43	44	45	46	47	48	49	50
Godthaab	—	84	131	118	122	104	45	105	86	68	130
Jakobshavn.	62	93	93	111	74	148	130	105	86	68	130
Smoothed means of both	70·5	87	106·2	129	109	109·2	101·2	95·7	86·2	88	109
Smoothed sunspots	54·4	40·2	23·9	15·1	20·2	39·1	65·3	95·6	110·7	95·6	76·3

The figures in Table II. speak for themselves.

To corroborate this law by further observation will necessitate a prolonged sojourn in some region *north* of the maximum auroral zone, and Greenland appears to be almost the only region where this could be done in the absence of a regular Polar expedition.

E. DOUGLAS ARCHIBALD

Tunbridge Wells

On Cases of the Production of "Ohm's (or Langberg's) Ellipses" by Biaxial Crystals

IN examining the macle crystals of potassium chlorate, which are so extremely common in the ordinary crystallised salt, I have found that all those which consist of two hemitrope plates only, nearly equal in thickness, give the above-mentioned secondary interference-curves when placed in homogeneous convergent plane-polarised light.

This result is no more than we should expect if the crystals were uniaxial, as Prof. Langberg showed (*Pogg. Annalen Ergänzungsbd.*, I, 540) many years before the curves were independently discovered by Prof. G. S. Ohm (see NATURE for November 27, 1884, p. 83). But potassium chlorate is a biaxial crystal, the angle included by the optic axes being $28^{\circ} 30'$ (determined in olive oil), and I do not find that the production of the curves in such crystals has been hitherto noticed.

The plane of the optic axes, however, makes so large an angle, viz. $38^{\circ} 30'$ (as determined in olive oil), with the normal to the surfaces of the plates in which potassium chlorate usually crystallises, that the isochromatic curves in the vicinity of this normal belong to a very high order, and do not sensibly differ from portions of circles of large radius. Thus in a macle, in which the crystallographic position of one of the components differs by 180° from that of the other, the planes of the optic axes make equal angles of $38^{\circ} 30'$ with the normal on opposite sides of it, and so the conditions determined by Langberg for the production of the secondary ellipses are fulfilled. I have, in fact, made artificial twins of this kind by cementing together plates of the salt oriented as above indicated; and I find that they show the ellipses precisely as the natural macles do. Of course, in order to see them, the compound plate must be so placed that the plane which includes the normal and the two acute bisectrices makes an angle of 45° with the plane of polarisation of the light. In a good micropolariscope the four optic axes and portions of the lemniscates immediately surrounding them are visible at the edges of the field.

It is possible, but not common, to find crystals of potassium chlorate consisting of three plates nearly equal in thickness, the top and bottom plates being symmetrically disposed, while the intermediate one differs from them in crystallographic position by 180° . In such cases the secondary interference-curves are much more complicated, two sets of ellipses being generally visible, one on each side of the centre of the field (the exact position, of course, depending on the relative thickness of the plates, as Langberg has shown). One macle I have found to consist of five or six distinct plates, and the secondary curves produced by this are too complicated to be easily described.

I do not find any marked difference between the curves produced by the iridescent twins and those given by the ordinary macles. Many of the iridescent crystals show, when the plane of

¹ The figures are smoothed by the formula $\frac{a+2b+c}{4}$, where b is the figure for the epoch and a and c the preceding and succeeding figures.

symmetry is either parallel or perpendicular to the plane of polarisation of the light (the analyser being crossed), a few broad, black, curved bands crossing the main black band lying in the plane of symmetry, which are probably portions of the isochromatic curves of a very thin plate. But, on the other hand, some non-iridescent crystals show these bands, and some iridescent crystals do not show them at all. Also the iridescent crystals which reflect D light at moderate incidences show very perfectly the circular band described by Prof. Stokes (NATURE for April 16, 1885, p. 566, par. 9) as sharp black crescents, the horns of which nearly touch each other at the plane of symmetry.

Almost the whole of the ordinary commercial crystallised potassium chlorate seems to consist of macles; so that, in order to get a single individual crystal for examination, I have always had to cut away one component of a twin.

It seemed worth while to try whether other biaxial crystals would, when similarly combined, give similar phenomena. I took a crystal of barite (barium sulphate), the angle included by the optic axes of which is, according to Groth, 63° in air, and cut a plate of it in such a direction that the plane containing the optic axes made an angle of 53° with the normal to the surfaces of the plate. I then cut it in half and cemented one of the portions upon the other in a reversed position. The compound plate thus produced shows the secondary ellipses (which, however, are very nearly circles) in great perfection. I have also made similar compound plates of borax, nitre, and citric acid, and found them to give similar results.

H. G. MADAN

Eton College, August 24

The August Meteors

BETWEEN August 4 and 20, 174 shooting stars were recorded here in $16\frac{1}{2}$ hours of observation. These included about 37 Perseids, chiefly seen on August 5, 8, and 13, but the shower was not well observed owing to cloudy weather. The following are the chief radiant points determined from the paths registered :—

No.	Epoch August	Radiant α δ	Notes
1	... 16-20	$5+12$	Meteors bright, max. Aug. 20.
2	... 13	$51+58$	Perseids.
3	... 4-17	$292+52$	Near χ Cygni.
4	... 5-13	296 ± 0	On equator near η Aquilæ.
5	... 5-20	$317+22$	Meteors slow and faint.
6	... 8-17	$318-9$	Slow, S.W. of β Aquarii.
7	... 15-17	$328+27$	Slow, faint.
8	... 11-15	$329+8$	Slow, bright, E. of ϵ Pegasi.
9	... 16-20	345 ± 0	Rather swift, bright.
10	... 8-20	$345+53$	Very swift, short.
II	... 16-20	$351+38$	Swift, E. of σ Andromedæ.

Many other shower centres were less distinctly shown. Nos. 4 and 9 fall exactly on the equator, and were sharply defined.

As to the shower of Perseids on August 10, I believe it was more brilliant than usual, though I made no regular observations on that night this year in consequence of overcast sky. Many meteors were, however, noticed in the clear spaces which now and then occurred, and judging from the frequency of the apparitions the display was a fine one. As to the duration of the shower it was still visible, though very feebly, on August 20, for I registered 2 undoubted Perseids during a watch of $3\frac{1}{4}$ hours, when 31 meteors were recorded.

With regard to the minor displays of this epoch they are more remarkable for their number than for individual intensity. The most active of these radiants, as recently observed, was No. 10 at $345^\circ + 53^\circ$, which supplied about 10 meteors, but the rate was less than one per hour, so that it cannot be ascribed much importance.

W. F. DENNING

Bristol, August 25

Disinfection of Sewers

IN the last number of the *Lancet* (August 15, 1885) I have read of the measures taken by the Metropolitan Board of Works for the deodorising and disinfecting of London sewers. Between 30,000 to 40,000 tons of sodium manganate and from 10,000 to 12,000 tons of sulphuric acid are daily poured in the London sewers.

By what experiments has it been ascertained that the quantities of disinfectants used are sufficient, and how is it proved that the sewers have been properly disinfected?

I need not point out the difference between the deodorising and the di-infecting of sewage. The latter may be perfectly deodorised, and yet be quite adapted to favour the vegetation of bacteria.

The oxidising and deodorising action of sodium manganate cannot be sufficient to prevent bacterial life, unless when the salt is present in large quantities. Considering the enormous volume of London sewage, it is not to be believed that even such a vast amount of manganate as 40,000 tons *per diem* would suffice to destroy bacterial life in the sewers.

The adding of sulphuric acid to the manganate must certainly enhance the disinfecting action of the latter. Only, I do not understand why the quantity of sulphuric acid is relatively so small in comparison with the quantity of manganate. I do not see why manganate should be used at all when sulphuric acid, a more powerful and less costly disinfectant, can be used alone.

It is well known to all who occupy themselves with the cultivation and study of bacteria that these micro-organisms do not grow well in acid media, and that the addition of acids, especially of mineral acids, checks their growth completely.

It can be said that the antiseptic action of acids is of household knowledge, for vinegar is constantly used in the preservation of animal and vegetable products. That mineral acids have a greater disinfecting action than vegetable acids is also well known, unfortunately even by dealers in vinegar, who give durability to this condiment by the addition of a tiny proportion of sulphuric acid.

It is probable that pathogenic bacteria, even more than the bacteria of ordinary fermentations and of putrefaction, are in need of alkaline media, and therefore are more sensitive to the action of acids. In the animal body bacteria invade those fluids and tissues where the alkaline reaction prevails; and it is proved that the germs of disease are easily spread by milk, a liquid generally alkaline. Moreover, it has been proved by experiments on some pathogenic bacteria that gastric juice, although of so slight acidity, easily, and sometimes effectively, checks their development.

Sewage contains all the elements necessary for the nourishment of bacteria, and its alkaline reaction renders it very favourable to their growth and preservation. Disinfection means the destruction of existing bacteria and preventing the development of newly-born bacterial germs. Therefore I am persuaded that the cheapest and more simple method for effectively disinfecting sewage is to render its reaction *permanently acid* by the addition of a sufficient quantity of mineral acid.

There are of course disinfectants far superior to mineral acids in antibacterial energy. But they are generally costly substances, that cannot be applied to the disinfection of such an enormous quantity of matter as the sewage of a town. As for cheap disinfectants, such as ferrous sulphate, ferric chloride, sodium manganate, their action is inferior to that of mineral acids. Especially of the two former it can be said that their deodorising action is due to their saline constitution, and their disinfecting action to their acid reaction.

The great difficulty in extensive disinfections is to ascertain if the disinfection has been complete—i.e., if the substance disinfected has been rendered unfit for the development and preservation of bacteria. Even laboratory experiments, to ascertain the *minimum* of disinfectants necessary for the destroying of bacteria, are not easily conclusive. But, in using acids, the disinfection can be considered complete when a permanent acid reaction is obtained.

I do not believe the quantities of sulphuric acid poured in the London sewers sufficient to give a permanent acid reaction to the sewage. Disinfection must be done completely, or not at all: there are no half measures in disinfection. Therefore I maintain that the London disinfection is useless, and the sewage remains likely to become the culture fluid of infectious germs, unless the sewage is rendered permanently acid. All the sodium manganate added to a sewage that remains alkaline, gets decomposed; the manganese precipitates as sulphide, or is deposited in combination or mixture with the organic sediment. The sewage will thus be cleared and deodorised for a while; but it still contains in solution all the elements necessary for the nourishment of Bacteria, and is still favourable to their growth and preservation. The disinfecting action of sodium manganate would avail only if large quantities of the salt remained dissolved in the sewage, over and above of the quantities decomposed in deodorising and clearing the putrid fluid.

It might be objected that, even if mineral acids stop the